January, February, and March are bright yellow; upon a second question, 'shining white yellow.'

April is blue, 'the shade ladies call French blue.'

May, light yellow, 'not at all like January.'

June, bright green.

July is glaring yellow; and *August*, orange.

September is golden brown; *October*, dark brown.

November is 'indiscriminate gray. I cannot exactly describe it: it is like lead color.'

December is gray.

This case appears to me sufficiently different from any of those mentioned by Galton to deserve special notice.

It would be very desirable, I think, to make a systematic investigation of the influence of heredity on such associations of color and form. Could not the Psychical society undertake such work?

CHARLES S. MINOT.

Boston, July 22.

Maxwell's demons.

Sir William Thomson has shown that since work is readily converted into heat, while heat is never wholly transformed into work, or in fact into any other form of energy, there must continually take place what Tait calls a *degradation* of energy; while its *dissipation* is pronounced to be the inevitable consequence of certain laws, connecting heat and work, established by thermodynamics.

Maxwell has pointed out that one of these laws is by no means a necessary truth ['Theory of heat,' chapter xxii., Limitation of the second law of thermodynamics]. Theory shows, that, in what is called a state of uniform temperature, some of the molecules of a body have by chance much greater velocities than others. If, therefore, as Maxwell says, we could suppose the existence of small beings, capable of following the motion of each molecule, and opening or shutting holes in a partition so as to allow the fastest molecules to pass through one way and the slowest the other, it might be possible theoretically, without expending any work, to separate a gas into two portions, — one hot and the other cold, — in contradiction to the second law of thermodynamics.

It seemed to me of interest to point out that what, as Maxwell has shown, could be done by the agency of these imaginary beings, can be and often is actually accomplished by the aid of a sort of natural selection.

When the motion of a molecule in the surface of a body happens to exceed a certain limit, it may be thrown off completely from that surface, as in ordinary evaporation. Hence in the case of astronomical bodies, particularly masses of gas, the molecules of greatest velocity may gradually be separated from the remainder as effectually as by the operation of Maxwell's small beings.

It is true, that, in overcoming the attraction of the central mass, the escaping molecules may be deprived of the whole or a portion of their velocity; but the transformation of heat into work marks the process still more distinctly as an exception to the second law of thermodynamics, which "asserts," according to Maxwell, "that it is impossible to transform any part of the heat of a body into mechanical work, except by allowing heat to pass from that body into another at a lower temperature" ['Theory of heat,' chapter viii.].

One might now dismiss the subject as a mere curiosity; but is it not possible that what may be called the *renovation* of energy plays an important part in the history of the universe? While philosophers, anxious to preserve their store of available energy, may speculate on the possible equivalence of renovation and dissipation, will not the scientist hesitate, without further examination, to extend the principle of universal dissipation from physical to astronomical phenomena?

HAROLD WHITING.

The classification and paleontology of the U. S. tertiary deposits.

In penning my protest (*Science*, June 12) against some recent geological and paleontological speculations of Dr. Otto Meyer, I had intended that it should represent my final words in the matter, inasmuch as the article under discussion appeared to me unworthy of exhaustive criticism. The appearance of instalment No. 2 of the same series (which, if any thing, is only more remarkable than No. 1), and a rejoinder to the first from Prof. E. W. Hilgard, constrain me to add a few additional paragraphs, more, perhaps, of a general than of a special character.

Professor Hilgard says, "I emphatically agree with Heilprin as to the impossibility of subverting the cumulative stratigraphical evidence to the effect that the relative superposition of the several principal stages — the Burstone, Claiborne, Jackson, and Vicksburg groups — cannot be otherwise than as heretofore ascertained;" and, further, "I recall to my mind that years ago I had occasion to repel a similar attempt, on the part of Mr. Conrad, to subvert the relative position of the Jackson and Vicksburg groups upon supposed paleontological evidence." It might appear, from the conjunction of these expressions, that the only evidence supporting the accepted superposition of the different members of the southern old tertiaries was of a stratigraphical character, and that the paleontological evidence was in conflict with that derived from stratigraphy. As a matter of fact, however, the paleontological evidence, whatever it may have been when Conrad first devised his scheme of classification, is, as we now know it, absolutely comfirmatory of the pregnant facts which the stratigraphy of the region presents; and, indeed, it would be difficult to find a region of similar deposits where it is more so. The absence or scarcity of forms of a distinctively old-type facies in the Vicksburg beds, and the introduction there of new forms whose equivalents or immediate representatives are known only from the newer horizon, are sufficient in themselves to establish the position. While it may be true, although this is far from being proven, that not a single one of the Vicksburg fossils is identical with species belonging to the typical oligocene basin of Germany, it is equally true that several of the species find their analogues or equivalents in the deposits of San Domingo, which are indisputably of post-eocene age; and whatever Dr. Meyer's own individual opinion may be as to the bugbear Orbitoides, and to its value as a 'leitfossil,' the keen appreciation of Hautken, Rupert Jones, Karrer, Fuchs, Suess, and Duncan has long since settled the question. It is amusing to have the forty-year old opinions of D'Orbigny and Edward Forbes referred to as authority on the value or no-value of certain fossil forms whose organization was barely known at the time that the opinions were rendered, and whose differences from other (distantly) allied forms were not even dreamed of. With singular perversity of purpose, Dr. Meyer fails to inform his readers that the American foraminifer whose merits are discussed by Professor Forbes, is confounded by that naturalist with a form which belongs not only to a distinct genus and family from Orbitoides, but to a distinct sub-order.

Aside from the testimony of the Vicksburg fossils

themselves, however, the dominating faunal features of the intermediate Jacksonian ought to have carried conviction, or nearly that, to the mind of any unprejudiced paleontologist. The Zeuglodontidae, represented (as generally considered) by the two genera Zeuglodon and Squalodon, are thus far positively known (in their earliest forms) only from late eocene or miocene (and oligocene?) deposits; and the only species of the former other than the American forms, and those obtained by Schweinfurth from Birket-el-Keroun (and recently referred by Dames to the eocene or oligocene horizon), is a member of the same group of deposits (the Bartonian) which in England correspond in position with the Jackson beds; i.e., overlie the Parisian (equal Claibornian). In that which relates to the oligocene (Orbitoide, Nummulite) rock of the peninsula of Florida, whose existence appears to give Dr. Meyer a considerable amount of anxiety, and which would better suit the requirements of the new theory were it cretaceous, our author need entertain no doubts: the rock is there, and has recently been found in several other localities which were not known at the time the mapping for my book was executed. No amount of chastising of Orbitoides will efface the testimony which it has unguardedly left behind.

I fully agree with Professor Hilgard as to the value of tracing derivative relationships between the species of the different formations, — a field of inquiry which I entered some years ago, but from which I have thus far drawn but barren fruit. In such inquiry it is necessary, however, to know the relative positions of the different deposits with which one is dealing, and not to proceed, as Dr. Meyer has done, from top to bottom, believing that top was bottom, and bottom top. Some curious evolutionary results might arise from this novel method of procedure.

For the rest, I need only reiterate my warning to geologists and paleontologists against the acceptance of the vagaries which are set forth in the two articles before us. Having given attentive study to the fossils from the region in question for a period extending over six years, and with the types of by far the greater number of species that have ever been described from the formation under my eyes and under my charge, I can say that those portions of Dr. Meyer's papers which relates to systematic paleontology are of about equal value with the geological, and clearly show that the author has not yet even found time to identify the numerous species which he is discussing. Pseudo-science of the kind to which we are here treated should be exposed. Angelo Heilprin.

Academy of natural sciences, Philadelphia, July 20.

The etymology of 'ginkgo.'

Mr. Lester F. Ward, in a note to his paper on the ginkgo-tree (*Science*, v. 495, June 19, 1885), says, "The orthography of this word ['ginkgo'] is not settled. Linné wrote 'ginkgo,' as did also, apparently, Kaempfer before him ('Amoenitat. exotic.,' 1712), and as all botanists since have done, and do still; but nearly all lexicographers reverse the consonants, and write 'gingko.' . . . In the supplement to Webster's dictionary the word is said to signify 'silver fruit;' and it would seem that the etymology ought to determine the orthography."

The first use of the word *ginkgo* occurs in Kaempfer's 'Amoenitates exoticae,' p. 811, where he says, "杏銀 *Ginkgo*, vel *gin an*, vulgo *itsjò*. Arbor nucifera folio adiantino." And then he adds a page of detailed description, and a page of figures of the leaves and fruit. He gives the two Chinese characters that are still used for the fruit in Japan: they are pronounced by two different methods, according to two provincial pronunciations brought to Japan long ago, and corrupted there, — either *ginkĭyoo* (not the common English *oo*, but each *o* long, or as in 'oolite;' and the *g* as in 'give,' of course), or, much more commonly, *ginnan;* and they mean 'silver apricot,' or 'silver almond.'

It is plain that Kaempfer's *ginkgo* was a misprint for *ginkjoo;* since the second character (杏) is also given by him on p. 798 for the apricot, and transliterated *kjoo*, — a very reasonable way to write it, with the German sound of *j*, and the long *o* doubled, as actually pronounced. Undoubtedly, the last syllable of the word was written in the same way at p. 811; but, in printing, it became *kgo*, and the error has been sacredly perpetuated until the present time.

The word *ginnan* (the first *n* is doubled in pronunciation) is likewise misprinted, on p. 812, 'ginnaṅ.'

Instead of *ginkiyoo*, or *ginnan*, the name of the fruit, the tree is called in Japan *ichoo* (two long *o*'s, as before, not the English *oo*, but the *ch* as in English), and that is what Kaempfer writes *itsjò*.

Thunberg ('Flora japonica,' 1784, p. 358), probably guided by his own ear, in amendment of Kaempfer, writes the name *ginko*, which represents a third less common Japanese pronunciation of the second syllable, *koo*, with two long *o*'s; and he speaks of the great size of the tree, comparing the thickness of the trunk to oaks. Possibly the evident errors of the Linnean name in spelling, pronunciation, and meaning (signifying the fruit rather than the tree, though Dr. Williams's dictionary says the same name is in China given also to the tree; and it is in Japan, too, sometimes given to the fruit-bearing sex of it), may be considered strong arguments in favor of the name 'Salisburia;' or, perhaps better, in favor of Thunberg's reform of the orthography — if that be not treading on altogether too holy ground.

Kaempfer pointed out the resemblance of the leaf to Adiantum, not only on p. 811, but again in the detailed description on the next page.

My copy of Kaempfer has an old manuscript note, as follows, — "1753. See this plant in Mr. James Gordon's garden at Mile End, London," — showing that the tree was very early introduced in Europe.

The tree sometimes grows to a very large size, and there was one about five feet in diameter in my garden at Yedo, on high ground; but damp soil is said to be its preference. The juice of the thick pulp outside the nut is very astringent, and is used in making a somewhat waterproof, tough paper, and a preservative black wash for fences and buildings. The meat of the nut is cooked and eaten.

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THE RECENT LAND-SLIDE IN THE WHITE MOUNTAINS.

Between Jefferson and the well-known Fabyan House, in the White Mountains of New Hampshire, is an oblong elevation of thirty-six hundred feet above the sea, known as Cherry Mountain. It is about seven miles in length